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Date: 22 Dec 2014

Thank you for this opportunity to review Dr. David Jay's thorough review of the hydrodynamic and sediment transport modeling performed by AnchorQEA (AQ) for the LWG. His knowledge of the Columbia and Willamette Rivers contributed substantially to his review of AQ's modeling system. I have added my comments (italicized and bold) below following select portions of his review (that I have enclosed in quotes). To make it easier to follow the text, I have left his section titles in this memorandum. I hope this format that I used for my review is acceptable.

One general comment I have after reviewing Dr. Jay's review is that no previously conducted modeling study at any other contaminated sediment Superfund Site is as comprehensive or advanced as recommended in his review. The three main reasons for this are the limited time available during the RI/FS to perform the modeling, limited available data, and limited or no funding to collect more data. Thus, the modeling study performed by AQ is based on a modeling framework that recognizes these limitations and makes simplifications and assumptions to accomplish the study within the scheduled time frame and for the available budget. This general assessment is based on the experience I have gained from performing a few modeling studies myself as well as from reviewing many RI/FS modeling studies for EPA (at sites in all regions but Region 7) over the past 25 years.

Nevertheless, as I commented on in previous reviews and model assessments I did for you and Chip, there are some major shortcomings in AQ's modeling framework. Dr. Jay identified many of these same shortcomings in his review.

Finally, many comments and recommendations made by Dr. Jay on various issues are repeated (and appropriately so) in different sections of this report. I attempted to limit my comments on these issues to the first time they are made in the review.

**I. Context**

Conceptual model of physical and sedimentary processes:

1. Important conceptual issues discussed in the CSM: the above physical CSM raises several important points:

    a) *"Complex circulation patterns associated with Multnomah Slough*: Given a potential for accumulation of contaminants related to complex circulation in this area and low computational cost, it is inexplicable that the decision was made not to extend the Columbia River grid domain at least to a point downstream of the intersection of the Columbia River with Multnomah Channel near St Helens, so that these circulation processes could be better modeled."

***I completely agree with this comment. In 2004 (if my memory is correct), Eric Blischke asked me to review the first hydrodynamic modeling of the Lower Willamette River and Portland Harbor that was performed by Tetra Tech, Inc. as a subcontractor to West Consultants. The grid developed by Tetra Tech stopped at the confluence of the Lower Willamette and the Columbia, i.e., it did not include any reach of the Columbia River at all. The highest priority comment I made in my review was that a sizeable reach of the Columbia needed to be included in the model grid so that the interact of the two rivers, especially during high flows in the Columbia when backwater from the Columba extends into the Lower Willamette, could be properly simulated. As does Dr. Jay, I said the grid in the Columbia should extend in both the upstream and downstream directions to locations where USGS gaging stations were located. The importance of representing this reach of the Columbia in the Lower Willamette River grid was driven home to me during the CSTAG visit to this site in ~ 2003. We took a jet-ski boat ride down to the Columbia, and even from the limited view available from the boat, I saw the enormous circulation gyres present in proximity to the confluence. There is no way to correctly simulate this or the Columbia backing up into the Lower Willamette using either Tetra Tech's initial or revised grid (which is the grid that AQ used in the modeling study they performed. Approximately one year after I submitted my review to Eric, he sent me the draft final hydrodynamic model report submitted by West Consultants to the LWG to review. Unfortunately, there was no contact between myself, the EPA team or West Consultants during this process. As stated above, the change Tetra Tech made to the grid (i.e., the addition of just the extremely short reach of the Columbia) was not acceptable in my opinion, and I am fairly certain I once again noted that in my review of the draft final report. I never heard back from Eric until approximately one to two years later when he asked me to review West Consultant's draft sediment transport modeling report. No changes had been made to the revised grid per my suggestion. When AQ took over the modeling study from West Consultants, they decided to use the revised grid and not extend the length of the grid in the Columbia.***

    b) *"Importance of bedload*: The physical CSM also emphasizes the importance of bedload transport, mentioning that about half the sediment transport from upstream into the Study Area (Rm-1.0 to 11.8) occurs via bedload, and arguing that a downstream decrease in bedload is important to deposition in the Study Area. It is then extremely puzzling then, that the decision was made in the RI/FS (Appendix La) not to model bedload. This appears to be a major omission that calls into question all results based on the sediment transport modeling."

***I completely agree with this comment. This was one of the major points I made to Chip following a presentation that AQ made on the sediment transport model they were developing. I think the presentation was either in the summer of 2008 or 2009. I again emphasized this deficiency in AQ's modeling framework when I reported on the review of AQ's model code, input files, etc. that I performed two or so years ago.***

2. <u>Conceptual issues not discussed in the CSM</u>: There are several important conceptual issues not discussed, or inadequately discussed, in the physical CSM. These include:

   a) *"Rain-on-snow winter flooding*: These floods are particularly important, because flows rise rapidly and the supply of fine sediment from upriver is large, leading to the potential for erosion (and export) followed by deposition. The Willamette usually rises faster than the Columbia, but the erosion potential of some winter floods is probably reduced by Columbia River flow management that causes artificially high water levels. Moreover, the fine sediment supply associated with rain-on-snow floods may differ from that which occurs under other conditions."

***I agree that this issue should have been included in the CSM, and the ability of AQ's hydrodynamic and sediment transport models to accurately simulate this type of event should have been performed.***

   b) *"The importance of tides*: The Willamette is an estuary (tidal to Oregon City), though an unusual one because of the absence of salinity intrusion. Tides are somewhat larger than in the adjacent Columbia River. They are largest during periods of low river flow, but are damped greatly by high flows in either of both rivers. Tidal currents are likely fairly weak, but that does not mean that they have no role in sediment transport. There is also a flood-dominant asymmetry in the tidal currents (at least most of the year) which may facilitate landward transport. It should be demonstrated that the numerical circulation model EFDC is correctly reproducing tides and tidal currents."

***Minor point, just academic in nature: The Lower Willamette River is part of the Columbia River estuary because astronomical tides propagate upstream to Oregon City under normal flows. I do agree that AQ should have demonstrated that their hydrodynamic model was capable of correctly simulating tidal flows in the Lower Willamette.***

c) *"Baroclinic processes*: Although there is no salinity intrusion into Portland Harbor, this does not necessarily mean that there are no stratified flow processes." "These stratified flow processes should be assessed through observations, to see if they need to be modeled, but there appear to be insufficient data available to accomplish this."

***I disagree with the need to do this. During high flow events, this relatively shallow (even in the navigation channel) river would be completely well mixed. During low flow events, there might be a small temperature difference over the water column, and maybe even between the two rivers; but if so, it is my opinion that the density driven transport would be minimal in such a highly advective river. To fully address this issue would require a very detailed and time consuming data collection and analysis program.***

d) *"Importance of extreme events*: Unfortunately, the flood with the best available data [December 1964; Waananen et al., 1970, 1971] has not been modeled."

***I do not know how detailed an investigation West Consultants/Tetra Tech performed while setting up, calibrating and validating their hydrodynamic model. Testing of their model by simulating the data-rich 1964 flood would have been ideal. I no longer have the copy of the West Consultants/Tetra Tech hydrodynamic modeling report that I reviewed in ~ 2005, so I cannot look to see if this flood event is mentioned in their report or not.***

e) *"Flood styles*: Willamette River floods occur in winter. Jay & Naik [2011] argued that there are three styles of winter floods in the combined Columbia and Willamette River system: i) Western sub-basin floods with extensive snowmelt, like February 1881, 1890, 1894 and 1955; ii) combined Interior and Western sub-basin floods like January 1881, 1964 and 1996; and iii) Western sub-basin floods without extensive snowmelt like 1909, and 1923.[1] These events likely have different signatures in terms of their influence on Portland Harbor sediment supply, erosion and deposition. Given the importance of floods in Portland Harbor sediment dynamics, this issue should be considered in the conceptual model and explored via numerical modeling."

***I agree that this issue should have been considered in the CSM. While the proposed numerical modeling investigation of the differences in the hydrodynamic and sediment regimes generated by the three styles of winter floods would have been informative, it is in my opinion beyond the scope of a typical RI/FS level modeling study. One reason for this is that such an investigation would require the collection of an enormous data set during at least one of each of the three types of floods. However, it would definitely be a worthy research project for a MS student.***

f) *“Climate change impacts*: Modeling is carried out for a 45 year period from 1979. However, no consideration has been given to how climate change has altered and is altering the Columbia and Willamette Rivers hydrological cycles, and sediment supply. Future floods may (or may not) be different from those over the last century, and the impacts of such changes on Portland Harbor contaminant stability need to be considered.”

***When the modeling study was performed, there was no Agency dictate to include possible climate change impacts in the modeling study like there is today. Even today, we are struggling to define realistic (i.e., possible) climate change induced scenarios that should be used in numerical modeling studies to determine their possible impacts on the hydrodynamic regime, and therefore the transport of sediments and associated contaminants, of the water body being modeled.***

3. “Recommendations: In terms of models, it is particularly vital to determine whether baroclinic circulation processes significantly affect Portland Harbor. If they do, the present generation of 2D (horizontal) models are likely inappropriate. Even if a 2D hydrodynamic model is appropriate, model verification has also not been adequate, and this problem should be corrected.”

***Complete or what a reviewer might deem an adequate model verification/validation is seldom achieved in these types of modeling studies because of a) inadequate existing data, and b) lack of funds to collect the necessary type and quantity of data. For these reasons, I have always used the phrase partial model validation was performed or attempted in the modeling studies I have performed.***

**II. Adequacy and use of oceanographic data**

1. Data available and missing: The data available for numerical circulation model calibration are:
2. “Recommendations: The available data set is not sufficient to allow a thorough calibration and validation of hydrodynamic and sediment transport models of Portland Harbor. Without proper calibration and validation of these models, it is not possible to accurately model contaminant transport, or to defend an analysis of remediation alternatives based thereon. Time series ADCP data should be collected at key locations in the system, time series of temperature and turbidity data should be collected (and for the turbidity data, calibrated with water samples), to determine whether baroclinic processes are important and to better understand suspended sediment distribution and transport. Other measurements, particularly time series measurements covering floods, should be made. Long-term moorings are needed for this purpose.”

***The many recommendations throughout in this document for collecting additional data are well intentioned, because modelers are never given a data set that would allow a thorough calibration and validation to be performed, but not realistic/possible in this case (or at any of the contaminated sediment Superfund Sites where I have worked). The inadequate data set is one of the reasons why a detailed sensitivity analysis is typically performed as a component of a modeling study. It also serves to put bounds on the uncertainty associated with long-term model simulations.***

**III. Understanding historical trends and climate change**

1. Historical trends and climate change:
2. "Recommendations: As noted above, climate change impacts and possible human alterations need to be considered in analyses of the system and design of remedial alternatives. The historical trajectory of the system needs to be understood in order to realistically predict future trends that may contaminant dynamics."

***While I do not disagree with this recommendation, it is, at least in my opinion, not possible to do within a RI/FS for the reasons given above as a significant amount of research would have to be performed to determine a probable "historical trajectory of the system".***

**IV. Analyses of Model Bias and Uncertainty**

1. Climate modeling and superfund modeling, similarities:
    a) *"Error propagation*: There is a need to assess the propagation of errors and uncertainty from one model to the next. This is vital in the superfund case, because the remedial alternatives cost very different amounts. Accordingly, there is a need to determine whether the results of these alternatives can be meaningfully distinguished. This can only be done if propagation of errors from one model to the next is assessed."

***While I agree in principle with this comment, performing the recommended error propagation analysis would be very time consuming and costly because of the long simulation times associated with sediment and contaminant transport modeling. Regardless, this analysis should be performed if the results from the modeling studies were going to be used in an absolute sense, e.g., determine how many years it would take for COC concentrations in surficial sediments or in fish to decrease to acceptable levels. At this time, long-term modeling studies are typically only used to perform a relative comparison between the chosen remedial alternatives. I will also state definitively that results from long-term modeling studies should ONLY be used to perform this relative comparative analysis.***

2. An inadequate framework for analysis or uncertainty and bias has been used:

    c) *"Accumulation of errors over time*: …Thus, an ensemble approach to long term prediction is needed to reduce uncertainty from all causes."

***Once again, while I agree in principle with this comment, it is not computationally feasible from a schedule or budget standpoint to perform the recommended ensemble approach to long-term prediction.***

3. "An approach to uncertainty analysis: Summarizing the above, correct evaluation of the impacts of biases and uncertainty propagation is vital to the integrity of the Superfund process. Also, it is best modeling practice to treat propagation of bias/uncertainty through the chained models as an aspect of the model architecture, not as an a posteriori addition [e.g., Reckhow, 1999; Reckhow and Chopra, 1999; Malve et al., 2005; Arhonditsis and Brett; 2004; Borsuk et al., 2004]. This is particularly important in the modeling of ecosystem and biogeochemical processes for which exact equations cannot be formulated, so that any model is, by necessity, statistical or heuristic [Kawamiya, 2002]. Aguilera et al. [2011] recently reviewed the field, described a variety of Bayesian approaches, and provide 76 references to applications in water and water resources. Chen and Pollino [2012] provide additional references and define the current state of best practices."

    "The above statements suggest an approach to long-term predictions that would be very useful in the RI/FS process. Rather than providing deterministic predictions from a 45-yr simulation for each remedial alternative, a range of outcomes should be provided that recognize not only accumulated modeling errors and biases, but also uncertainties in future climate and hydrologic conditions."

***Once again, while I agree in principle with this comment, it is not computationally feasible from a schedule or budget standpoint to provide the recommended range of outcomes. This could be done, but it would at least double the length of time and budget required to perform contaminant transport and fate modeling studies. As stated previously, "deterministic predictions from a 45-yr simulation for each remedial alternative" should only be used for relative comparisons amongst the proposed remedial measures.***

4. *Recommendations*:

**V. Hydrological Analyses and the 100 year flood**

1. Hydrologic analyses and the 100-year flood:

2. *"Recommendations*: A 100-year flood volume of about 500,000 cfs is realistic and should be adopted. Given climate change and a highly non-stationary Willamette River flow record (see Appendix I), it is difficult to estimate 500-yr flood magnitude. Anything

smaller than the 1861 flood is clearly unrealistic. For lack of a better alternative, the 1861 flood could be adopted. Also highly relevant is the issue that the range of past and likely future Willamette River flood styles (and their likely distinct sediment input patterns) should be modeled."

***I agree with the recommendation regarding the 100-year flood magnitude.***

**VI. Hydrodynamic Modeling**

1. Hydrodynamic Modeling -- Importance:
2. Grid resolution and refinement: Among the consequences of inadequate horizontal grid resolution are the following:
    a. *Resolution of small-scale features on the bed*:
    b. *"Effects of elongate grid cells*: The aspect ratio of 200m to 25m or 8 is quite large, with associated poor numerical properties. In physical terms, a 200m long grid cell is likely to include quite variable depths and not represent processes well."

***I agree with the second sentence. The effect of the aspect ratio of grid cells on the numerical solution is well known, but has not been quantified for this modeling study.***

c. *"Numerical dispersion*: The larger the grid cells, the more numerical dispersion must be included into the model to provide stability. This issue has not been discussed – it should be."

***The discretization of the spatial domain and the use of approximate difference equations to approximate the solution of the governing nonlinear partial differential equations introduce numerical dispersion into the approximate solution. As Dr. Jay states, the larger the grid cells, the more numerical dispersion is included in the approximate solutions to the discrete difference equations.***

d. *"Representation of remedial alternatives*: The present limited grid resolution limits the accuracy of mapping of some remedial alternatives onto the model, decreasing the accuracy of related simulations."

***I agree with this comment.***

e. *"Resolution of structures*: There are numerous bridges across the Lower Willamette that cause locally strong currents and scour, and represent a form of flow resistance. While EFDC does allow their effects to be treated via a bed vegetation algorithm, the limited model resolution means that this cannot be done very accurately."

***The significance of not resolving the structures in the numerical grid would have to be numerically investigated. I do not think not representing the structures (e.g., bridge pilings, piers) will significantly impact the solutions of the sediment and contaminant transport models.***

f. *"Analysis of grid resolution*: It is good modeling practice to carry out a grid resolution analysis to determine what grid resolution is required [Roach 1994 and 1997]. … No such tests were carried out as part of Portland Harbor modeling. Also, grid statistics associated with grid aspect ratio, distortion and smoothness should be provided."

***I do not know if grid resolution tests were performed by Tetra Tech or AQ or not. How does Dr. Jay know that they were not?***

3. Grid areal coverage:

4. Boundary conditions -- elevation:

5. Boundary conditions – stream flow:

6. Lack of representation of baroclinic processes: "Because the hydrodynamic model is 2D rather than 3D, it cannot represent circulation processes related to horizontal and vertical density differences. It is also limited in the realism of its sediment transport calculations in deep water – often 10-14m in Portland Harbor. Even moderate levels of density stratification strongly affect the vertical structure of velocity and sediment profiles, and horizontal density gradients drive currents. Also, integrated sediment concentrations of ≥2gr/l during the December 1964 flood [Waanenen et al., 1970] are indicative of sediment stratification strong enough to alter vertical mixing, and velocity and sediment profiles. While the sediment concentrations during this flood are likely a rare circumstance, it is also very important to model such events."

***The degree of vertical stratification of the suspended sediment during an event as infrequent as a 500-year flood would depend on the variation of the grain size distribution of the suspended sediment. My judgment is that fine-grain cohesive sediment and very fine to at least medium sand size sediment would be fairly well mixed over the water column in such a large advection dominated flow. Sediment coarser than this would most likely become increasingly stratified with increasing size. The significance of the vertical stratification on the vertical velocity profiles would depend on the percentage of the total suspended sediment***

***load that the coarser sediments possess. The latter would have to be measured during a large flood of similar magnitude and used to verify the vertical sediment and velocity profiles predicted by a 3D sediment transport model. Assuming that these data do not exist, and thus it would not be possible to determine the effect of any vertical sediment stratification on the flow field, it is not an unreasonable assumption to assume that the sediment being carried in suspension during a flow as enormous as a 500-year event is vertically well mixed.***

7. "Calibration issues: There has been no systematic comparison of modeled and observed water levels. Comparisons of means is not too the point in a tidal water way. No available report even states what tidal constituents are included in the model. A careful analysis of model's representation of both tides and river stage is needed. In particular, it needs to be demonstrated that tides decrease in the correct manner as flows increase, and that overtides vary in the correct manner with flow. Both tidal constituents and defined water levels (like higher high water HHW, mean water level MWL, and lower low water LLW) should be considered. … No moored ADCP time series data have been used in evaluating model behavior, even though such data have been available since 2003 at the Morrison Street Bridge. The velocity calibration rests on comparisons with lateral profiles on three different days. Again, no systematic comparison of the reproduction of tidal currents and mean flows has been attempted."

***I agree that the calibration of the hydrodynamic model was limited. I believe I commented on that in the review I performed for Eric.***

8. "Validation: Separate calibration and analysis periods are needed to fully validate the EFDC circulation modeling. Each period should be at least a year long and encompass both flood periods and low-flows."

***The specified, non-overlapping time periods for use in calibrating and validating the hydrodynamic model would be ideal. Sometimes compromises, e.g., the calibration period is a subset of the longer validation time period, have to be made due to lack of available data.***

9. Sensitivity analysis:

10. "Bed roughness: Appendix La indicates that bed roughness $Z_0$ has been set to a constant value, because theoretically based efforts to explain its variability were unsuccessful. While it is better modeling practice to use a constant $Z_0$ than to use bed roughness as a tuning parameter, the bed in Portland Harbor is quite variable, with areas of fine sediment and sandy sediments with bedforms. Thus, $Z_0$ should vary. After implementation of a better grid and a careful treatment of other aspects of the hydrodynamic model calibration, this issue should be revisited."

***It is very difficult to use a variable $Z_0$ throughout a model domain when data are not available in the portions of the water body where the values of $Z_0$ are different. Without such data, comparisons of simulated elevations and velocities versus data cannot be performed to be able to assess whether the spatially variable $Z_0$ values improve the model calibration or not.***

11. "Analysis of the 1996 flood: The 1996 flood event was used as a validation exercise. Unfortunately, all Columbia River tide gauges between Longview and Vancouver failed, emphasizing the need to place model boundaries in locations (Beaver and Bonneville Dam) where forcing and validation data are available. Predicted water levels were well above observed water levels, with a discrepancy at the Morrison Street Bridge of up to 0.8m. This indicates unsatisfactory behavior model performance and likely inaccurate predictions of sediment transport. However, few data are available for other parameters aside from water level. The reason for the poor modeling of water levels during the flood is likely related to the grid extent and the boundary conditions used."

***I agree with the assessment stated in the last sentence.***

**12.** Recommendations:

**VII. Willamette and Columbia River sediment loading**

1. "Willamette River sediment loading: Sediment supply from the Willamette River is a vital boundary condition for the sediment transport and fate and transport models. Only post-1973 USGS sediment concentration and load data for the Willamette River were used, with observations for days with flows up to about 200,000 dfs. This ignores the larger 1962-1965 daily data set that includes detailed observations for the December 1964 flood, including multiple observations on the days of peak sediment load. The 1964 flood had a peak flow of about 443,000 cfs; it is one of the four largest of the last century. Accordingly, the 1962-1965 data set is an important resource that should have been used. This data set also provides percent sand data, so that the sediment load can be correctly divided into sand and fines transport, and the fines load needs to be divided into silt and clay inputs. Additional problems include:
    a. *Hysteresis effects*: The rating curves derived in Appendix La do not consider sediment load hysteresis, though this is an important factor in the system. Typically, the sediment load is highest on the rising arm of the freshet, and this is an important feature of rain-on-snow floods. Willamette River sediment supply is analyzed in Appendix II.
    b. *Sediment quality*: The RI/FS modeled division of the supply between fines and sand is incorrect for high flows, in part because it does not consider the very

large supply of clay material, which is likely most prominent during rain on snow floods.

c. *Lower Willamette River deposition and erosion*: The sediment load measured at the Morrison Street Bridge does not represent the load to the Lower Willamette River, because Morrison Street Bridge measurements are affected by deposition and erosion between Oregon City and Portland Harbor. It is likely that the load during low-flow (depositional) periods is underestimated, while the load during high flow periods may be overestimated.

d. *Use for validation*: The correct use of the Morrison Street Bridge data and rating curve is for validation of the model predictions, not as a boundary condition, because the sampling is within the system, not at the boundary. This problem can only be remedied after collection of an appropriate data set at Oregon City."

***All these comments are valid ones that AQ should address.***

2. "<u>Columbia River sediment loading</u>: The Columbia River sediment load at Vancouver has been set, based on 1963-1969 data. This is a reasonable first step, but the percent sand has been underestimated. Information in Haushild et al. [1966] should be used to set the percent sand as a function of flow. Also, post 1973 USGS NWIS should be used, as was done for the Morrison Street Bridge."

***This is also a valid comment.***

**VIII. Sediment transport modeling**

"Moreover, the treatment of the bed and of fine sediment erosion, deposition and settling in the SEDZLJ module of EFDC are, taken together, extremely complex. The number of parameters is sufficient to allow the model to be tuned to correctly represent any particular event, but this does not mean that the model can provide accurate forecasts. Unfortunately, there are few Portland Harbor data available that can be used to objectively set these parameters, and essentially no data for validation of sediment transport predictions. Thus, sediment transport model parameters and results must be regarded as very uncertain, calling into question the fate and transport modeling based on the sediment transport modeling."

***I disagree with Dr. Jay's assessment of the sediment transport model used by AQ. SEDZLJ is more constrained then most sediment transport models because of the use of SEDFLUME data to specify the gross erosion rate as a function of the applied grain stress.***

There are several specific issues in the sediment transport modeling:

1. <u>Grid resolution</u>:

2. Vertically integrated (2D) model formulation:
3. "Choice of size classes: While there are presently insufficient data to make separate rating curves for fines and clay, these two should be distinguished for modeling purposes, because their behavior will be quite different. In summary, a sixth size class is needed, and load and water column size data need to be collected to support this distinction."

***A non-settling washload sediment class was used in the modeling.***

4. "Settling velocities: The settling velocity formulation for the four size classes of sand and gravel is conventional. For the combined silt and clay size class settling velocity is given by:

$$W_S = 3.3\ (C_1 G)^{0.12} \qquad (1)$$

where $W_S$ is in m/day, water column shear stress G is in dyne/cm$^2$, and $C_1$ is concentration of size class 1 in mg/l. There are several difficulties with the use of this formulation in the present case:

a) Gradients in shear: Horizontal gradients in shear are high in Portland Harbor, especially during high flow periods. Thus, as a parcel of water moves, the $W_S$ of its load may vary, according to (1). In systems with large spatial scales and slow motions (like most lakes and reservoirs), particles will have time to adjust to their changing environment. This may not be the case in Portland Harbor, and (1) which likely represents equilibrium behavior, may not be appropriate. Unrealistic results may occur both during high-flow periods and in times and places where tidal currents reverse, because shear will change rapidly in both cases.

b) Gradients in concentration: Horizontal gradients in concentration $C_1$ have not been estimated for Portland Harbor, but the same issue applies to these gradients as to shear gradients. Eq. (1) will be unrealistic if the predicted values of $W_S$ change more rapidly (due to advection to a different environment) than the particle field actually responds.

c) Value of G: The shear G is intended to be a water-column value, but bed skin friction shear stress $\tau_{SF}$ is used instead, because this is the only value of stress that is available in a 2D horizontal model. If the flow is approximately a channel shear flow, then the shear varies linearly with depth, being maximum at the bed surface and zero at the free surface (unless there is wind). Use of $\tau_{SF}$, which is a component of the bed stress, but not all of it, will mis-estimate the water-column; values may be either too high or too low (if bedforms are present).

d) Problems as slack water: During periods of weak river flow, currents do reverse in Portland Harbor, and slack water is time when sediments typically settle to the bed. The $W_S$ formulation in (1) prevents this from happening by taking $W_S$ to zero as the current slows. This is clearly unrealistic."

***I agree with all these comments.***

5. "Absence of modeled bedload transport: As noted above in the CSM discussion, other WLG documents emphasize the important role of bedload transport in bathymetric changes within the Study Area. Thus, it is very surprising to read in Section 2.1 of Appendix La that no bedload transport has been modeled. The reason given is that no formulation of bedload transport over a cohesive bed is available. This is a problem that should be dealt with, given the importance of bedload in the system."

***As stated previously, I agree with this comment.***

6. Vertically integrated formulation:

7. "Coupling of hydrodynamics and sediment transport: EFDC and SEDZLJ are not coupled in the sense that changes in bed elevation (due to deposition and erosion) predicted by SEDZLJ are not coupled back into the EFDC. Under most circumstances, this will not cause major problems in the modeling, and it is a useful simplification for long simulations. However, erosion may reach ~1m during severe flood events. This degree of erosion will change the hydrodynamics. The impacts of this simplification should be judged using fully coupled runs for comparison. Impacts of this simplification also need to be considered in sensitivity analyses."

***I completely agree with this comment, and identified this as one of the major limitations of AQ's modeling framework in the review of AQ's hydrodynamic and sediment transport modeling that I performed for Chip two or three years ago.***

8. "Sediment load time resolution: Given that Willamette River flow can vary by >2000$m^3$/s, 24-hr period, sediment load can by vary at least by an order of magnitude a day. Thus, sediment load input from upriver should be updated on the same schedule as the river flow – it is unclear whether this is being done at present, and the Phase 2 reports suggest that it is not."

***I agree with this comment.***

9. "Model validation: The validation of the sediment transport model rests entirely on attempts to reproduce observed 2003 to 2009 erosion and deposition patterns, a time

period without a major flood. The difficulty with this approach is that it is inherently ambiguous and incomplete. It is impossible to know, even if the bed changes are plausible for the time period, whether the right answer has been reached for the wrong reasons. For example, if a model and data agree that an area shows no net erosion or deposition over a time period, this does not make the model correct, because erosion and deposition cycles and events that profoundly affect contaminant transport may not have been modeled correctly. Further, the Willamette River sediment load is incorrect (Appendix II) and bedload transport has been neglected. Thus, it is likely that the model's success is based on incorrect parameterizations, calling into question its predictive ability. Given the difficulties documented above in the hydrodynamic and sediment transport models, it is vital that SEDZLJ water column transport predictions be tested against data. While further data collection is needed, there are readily available data sets that have not been used. One is the 2009-2014 USGS time series of turbidity at the Morrison Street Bridge. Acoustic backscatter data or ABS (better for coarser sizes) could also be obtained from USGS for the Morrison Street Bridge side looking ADCP from 2003-2014. Both time series should be calibrated, considering variations in both particle size and concentration."

***I agree with this comment.***

10. Data collection needs: Additional data collection should include:
    a. Concentration data from turbidity:
    b. Concentration data from ABS:
    c. Moored LISST data:
    d. Water samples:
    e. Sediment load at Oregon City:
11. The bed model in SEDZLJ:
12. Sensitivity analyses and error propagation:
13. Modeling of the 1996 flood:
14. Recommendations:

**IX. Fate and transport modeling**

1. Boundary loading of contaminants:

2. "Mapping of scenarios onto the grid: Section 5.3.1.1 of Appendix Ha indicates that the grid is too coarse to accurately map the remedial alternatives onto the bed. While this may not be the most serious problem associated with limited grid resolution, it is one of the issues."

***I agree with this comment.***

3. "Calibration period: The post 2003 calibration period does not have a really major flow event. While available data may require this period to be used, this still represents a limitation on the model capabilities."

***Unfortunately, it is a recognized but unavoidable limitation.***

4. "Sensitivity analyses: The uncertainty analysis does recognize the importance of sediment loading, but no other sources of uncertainty and bias associated with the hydrodynamic and sediment transport modeling are recognized. The result is that uncertainties are far higher than reported."

***I agree with this comment.***

5. 100-year flood event:
6. "Variability of natural conditions: For each remedial alternative, a spectrum of forcing scenarios should be considered, to determine how variable future outcomes may be, including effects of long-term changes in hydrology and climate."

***The results of the long-term transport and fate simulations performed during the FS are used for relative comparisons amongst the remedial alternatives. As such, the use of a spectrum of forcing scenarios is not necessary.***

7. Bed layering:

8. Recommendations:

**X. Recommended future analyses**

"There are a number of steps that EPA could take with existing data and models that would improve the understanding of the Portland Harbor Superfund site, its history, and relevant processes, and provide a better ability to evaluate model outcomes. These include:"

1. Historical data:
2. Water level data analyses:
3. Analysis of USGS data sets related to turbidity:

4. Bed sediment and sedimentology:

5. Sediment load analyses:

6. Analyses of water levels and bed stresses:

**7.** Multi-modeling:

***In general, I agree with a majority of the comments made in this section. However, some of these recommendations are beyond the scope (for the previously cited reasons) of a COC transport and fate modeling study performed as a component of a RI/FS.***